

February 9, 1959

Professor Frank Fenner
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Dear Frank:

Your letter of January 22nd was a happy item among the pile of mail that awaited my arrival at Stanford. Bravo!

It seems to me you have certainly made a sound approach to terminology. As to pock character, I think your alternative suggestion to use the symbol u for the non-ulcerating mutants may be more satisfactory. In principle, the symbol x⁺ meant the wild type rather than necessarily the presence of the trait 'x' but by and large it seems to me more convenient if the two criteria can be made to coincide, as you have done with the symbol u. It is not surprising that white mutants might also be minute and one simply has to be rather arbitrary in choosing the symbols. There would be nothing very much against your setting up another class of symbols for those mutants which are at the same time non-ulcerating and minute, for example 'um'. The virulence characters clearly will be the most difficult to cope with, especially in the interval during which they have still to be unravelled. I would be inclined to try to develop a terminology where the plus symbol was identified with high virulence. In this case the selection of a given allele as wild-type is found to be rather arbitrary anyhow. I have always found the host range terminology, where the symbol h⁺ meant a limited host range, to be rather confusing.

How be it, I do not see how you can go seriously wrong with any of the suggestions that you put in your letter. These developments really are most exciting?

We are of course looking forward to the rounding out of our research program, especially when Gus Nossal gets here this summer. I hardly need to tell you how eminently suited Gus might be for another appointment in the John Curtin School, provided only he could divest himself of the usual Australian attitude about Cambra. I am a little surprised to hear at nth hand that Steve does not think too much of the elective approach to the formulation of antibody formation. For my own part, I don't think that the chemical situation is as clear-cut as he might conclude himself and what I get from the analyses that have been published and also from Karush's work actually seems to me to fit very well the idea of differential sequence as a determinant of antibody specificity. Anyhow I have a paper in press in "Science" which elaborates these themes and attempts to put Burnet's proposals into somewhat more explicit form in the context of biochemical genetics.

It was a pleasure to hear from you and may I return all best wishes.

Yours sincerely,